in approximate evanescence an error of half an hour, or even an hour, is quite insignificant.

If these 24 cases be subtracted, the probable error in the time falls to 7m.

These retrospective calculations are distinctly better as regards height than those of the Indian Government, but are worse in the times. It is proper, however, to remark that better values of the tidal constants were used than were accessible in 1884.

The comparison between our predictions and the Indian ones made for the whole of 1889 by Mr. Allnutt, led to conclusions very similar to those just explained.

It may be concluded from these comparisons that highly satisfactory tide-tables may be made from our general tide-tables, when the tidal constants are accurately known.

If the general tide-table were computed in the full form, both for high water and for low water, it would cover 36 pages of rather small type. But at many places it may not be thought worth while to proceed to the full degree of accuracy. If the computation were made for every half-hour of moon's transit, and for every fortnight, the table would be reduced to 16 pages; and if high waters only were given, to 8 pages.

I believe, however, that this is not the direction in which abridgments may be best made, but that it would be more advantageous to drop some of the corrections. Thus the correction to the height, which depends on the longitude of the moon's node, and a certain small correction to the time, which depends on a portion of the equation of time (of which no mention has been made), might be dropped without much loss of accuracy. If the tables are still supposed to be half-hourly and fortnightly, these omissions might reduce them to 6 pages. At some ports a rough high water tide-table might even be contained in 2 or 3 pages, but in this case it might be necessary to add a warning of the possibility of sensible error in the times and heights of the lower high water.

The question of how far to go in each case will depend on a variety of circumstances. The most important consideration is, I fear, likely to be the amount of money which can be spent on computation and printing; after this will come the degree of desirability of a trustworthy tide-table, and the accuracy with which the tidal constants are known—for it is obviously useless to form a nominally accurate tide-table from inaccurate values.

My aim has been to reduce the tables to a simple form, and if, as I imagine, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, the principal object in view has been attained. G. H. DARWIN.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

SUFFICIENT time has elapsed since the dismissal of the International Congress to permit a deliberate view to be taken of the results of the meeting, and to determine how far the prospects of the photographic chart have been improved by the resolutions which the Committee has entered on its minutes. But there is the preliminary consideration, that it is now four years since the scheme was definitely conceived and resolved upon. Have those four years been profitably employed? Doubtless the initiatory steps, such as procuring the necessary instruments, and acquiring facility in their use, as well as a knowledge of their capacities, have occupied a longer time than was originally anticipated, but seen in the light of a fuller experience the completion of so many details in a shorter time was not warranted. It is a matter of legitimate congratulation that all the participating Observatories are adequately and similarly equipped, and that a work undertaken in hope, rather than founded on experience, is moving surely and certainly to a successful issue, and strictly on the lines originally contemplated. The early resolutions of the Committee were not guided by trial and practice, and it is fortunate that so little has had to be altered, and that the scheme in its integrity remains practically as it was conceived. The words with which Admiral Mouchez closed the Conference on the fourth day of debate happily described the situation, and the attitude of the various members of the Committee. "You have had to treat difficult questions," said he; "you have treated them without reference to any other thought than that of truth, and the progress of science. Divergences of views have exhibited themselves as to the means to be employed, never as to the end to be attained. These divergences were inevitable; they were even necessary; they have only made us see the questions in a clearer light, and have never disturbed the cordiality of your relations. The unanimity which has marked your final decisions is a certain pledge of ultimate success. It is in this conviction that I declare the Conference of 1891 closed, and the work of the photographic chart of the heavens commenced."

The keynote of one of these "divergences of views" was struck by the Admiral himself in his opening address, and the subsequent debate led to the exhibition of considerable excitement on the part of individual members of the Committee, which has been commented on elsewhere. It is well known that under the auspices of the International Committee it is proposed to effect two objects, each very desirable in itself. One of these, the one which really called the Committee into existence, is the photographic representation of the present condition of the heavens, in which shall be delineated stars which, under a certain definition, are called the fourteenth magnitude. The other, a more recent thought, is the preparation of a catalogue in which shall be exhibited the co-ordinates of stars down to the eleventh magnitude, on the scale of Argelander's zones. The question arose, as Admiral Mouchez pointed out, whether these two distinct and separate objects should be proceeded with *pari passu*, or whether, since a shorter time would be sufficient to furnish the plates from which the catalogue would be deduced, it was desirable that the data for the catalogue should be supplied before the more arduous undertaking of the chart plates should be commenced. Admiral Mouchez, in his opening address, clearly indicated the advisability of the latter method of proceeding, and adduced very excellent arguments for such a course. A catalogue, as he remarked, is immediately useful, and is a want among astronomers of the present day, whereas the chart is really intended for the benefit of future astronomers, who will value it the more in proportion to its completeness. Further, there are reasons to believe that the duration of exposure necessary to secure, on the negative, a black measurable image of a star of the fourteenth magnitude, will be much longer than has hitherto been anticipated, and that consequently a much greater expenditure of time will be necessary for the production of the plates than has generally been imagined. If, then, the two series of plates are obtained simultaneously, the shorter exposure plates will be injudiciously delayed, and the manufacture of the catalogue cannot be proceeded with till the chart plates of long exposure are completed—a course manifestly to the detriment of the present condition of astronomical science. On the other hand, if by co-operation the series of negatives of short exposure be secured in about two years, which is quite possible, the hands of astronomers will be greatly strengthened by the welcome which will be accorded to the catalogue, and they will be encouraged to undertake the more onerous part of the work. Additional experiments and information could be collected, and it is not too much to hope that some improvement in the preparation of photographic films might secure additional sensitiveness, and so facilitate and abridge the operations.

It is evident that here the lines of cleavage are saliently marked. It was inevitable that some members would feel themselves definitely pledged to the chart, and nothing but the chart; while others, looking to the applicability of photography to the rapid registration of stars, and the accuracy with which their places can be determined, would see greater immediate advantages to be drawn from the compilation of a catalogue, and would endeavour to divert the energies of the Committee into that particular channel.

The debate which followed was interesting, but turned mainly on the points indicated above. The Astronomer-Royal, who had not heard, but was probably acquainted with, the views of Admiral Mouchez, maintained the principle of the chart in its integrity, and this he could do the more boldly since he advocated but a very moderate exposure as sufficient to secure the impression of stars of the fourteenth magnitude. He admitted that, on nights when the sky was not wholly fine, it might be advantageous to take plates of short exposure, suitable for the catalogue, but that the general principle to be observed was, to take the short and long exposed plates, the one directly after the other, in order to avoid the frequent adjustment of the equatorial to the particular part of the sky to be photographed. Moreover, the Astronomer-Royal urged, and the force of the argument cannot be denied, that the Government funds had been granted with the special view of procuring the chart, properly so called, and that a breach of faith would be involved in urging forward any other work with instrumental means so provided, till that for which the grant was allowed was completed.

The proposal in favour of immediately proceeding with the catalogue plates found an able advocate in Dr. Gill, who, however, urged his entire devotion and loyalty to the completion of the chart as originally conceived. He reiterated the points that Admiral Mouchez had so clearly put in his opening address; and in addition, that it was undesirable to embark upon a work of unknown magnitude and extent, seeing that one ready to the hand, which equally involved the construction of a chart, called imperatively for their attention. Captain Abney had previously pointed out a method by which he believed the sensitiveness of the emulsion and the measurability of the images could both be improved, and Dr. Gill seized this point to urge on the Committee, with the weight due to his experience in this department of astronomical photography, the necessity of longer delay and further experiments. Other members of the Committee, with more or less originality, repeated these arguments, and in the hopelessness of arriving at a decision the President (M. Bakhuyzen) adjourned the debate, in the hope that reflection and informal interchange of opinions might disclose a *modus vivendi*.

The method pursued in this discussion has been dealt with at length because it is typical of the procedure that was adopted whenever it seemed impossible to arrive at a unanimous vote. In this case the expectation of the President was not disappointed. An informal meeting of several members of the Committee was held before the next *séance*, and the following resolution was drafted, which was carried without discussion and without dissent:—

"The work undertaken by the Congress of 1887 comprises two series of negatives made with different exposure: the permanent Committee, whilst recommending to observers to urge forward, with the greatest possible diligence, the execution of the negatives of the second category (negatives destined for the construction of a catalogue) is of opinion that they ought also to profit by the greatest possible number of fine nights to take negatives of long exposure for the first series (those of the chart)."

The resolution thus carried happily combines all sections of the Committee in a common work, and though the execution of either the chart or the catalogue was never really jeopardised, it is certain that both will go forward with greater alacrity and harmony on the part of all concerned.

In the early days of the scheme, members talked very glibly of the fourteenth magnitude, though not recklessly, for they were cautious to couple it with a definition, the accuracy of which may be now impugned, but which at the time sufficiently limited the acceptation. It was assumed, though possibly on insufficient evidence, that the exposure of a plate two and a half times that of a given exposure would insure the impression of one additional magnitude. This point has been submitted to further experiment, and it was shown at the Congress, principally by Dr. Scheiner, of Potsdam, that this law did not hold. The time required for the impression of stars brighter than the ninth magnitude is so short that there is a very great difficulty in determining it accurately. Dr. Scheiner had therefore, with a view of settling this question, carried out a series of experiments under various conditions—

(1) By employing plates of low sensibility.

(2) By covering the object-glass with a gauze screen having a very fine mesh.

(3) By reducing the aperture of the object-glass to nine centimetres, and employing both rapid and slow plates.

The result at which he has arrived after these experiments, the most complete that have been made, is that by multiplying the time of exposure by two and a half, a whole magnitude is never gained, but only a half or seven-tenths of a magnitude, and that consequently to obtain the fourteenth magnitude an exposure of *seven hours* would be necessary. Some objections may be raised to the method in which the magnitudes were determined by Dr. Scheiner, particularly to the result deduced from counting the stars. On a plate which he had exposed for eight hours he found impressed only 5689 stars, whereas it might have been anticipated that there would have been 10,566 stars. The argument drawn from the numbers counted might be valid if applied to a large portion of the sky, but on the small district covered by one plate various causes might operate to invalidate the law. Taking Dr. Scheiner's results, however, as they stand, the chart of the fourteenth magnitude is rendered impossible, and the Committee felt it necessary to adopt a method of proceeding which would render the chart not only possible, but should secure uniformity, and also permit of the scheme being carried out within a reasonable time.

By the kindness of the Paris authorities in forwarding early proof-sheets of the *procès-verbaux*, we are enabled to put before our readers a rigorous translation of the resolutions which the Congress has adopted, with unanimity, to this end.

"With the view of enabling observers to pass in a uniform and certain manner from the ninth magnitude of Argelander to the eleventh, which it is sought to obtain on the negatives destined for the catalogue, a Commission (consisting of MM. Christie, Henry *frères*, Pritchard, and Vogel) will distribute among the participating Observatories, screens, having a metallic webbing, absolutely identical for all the Observatories. These screens, placed in front of the objective of the photographic telescope, will diminish the brilliancy of a star by two magnitudes, and in the determination of the diminution of magnitude, the Commission will adopt the coefficient 2·512 for the ratio between two consecutive magnitudes."

Since there is no difficulty by various photometrical methods in determining the magnitude of a number of stars of the ninth magnitude, every Observatory will, with strict uniformity, be able to insure on its plates the impression of stars of the eleventh magnitude. And, in order to insure a reasonable limit of exposure for the chart plates, and to avoid any ambiguity which the employment of such a term as the fourteenth magnitude might produce, the Committee has further passed the following resolution :—

"The Committee indicates forty minutes as the duration of exposure for the negatives destined to form the chart, in the mean atmospheric conditions of Paris, and with plates prepared by the firm of Lumière, which are at present in use at Paris.

"The Commission mentioned above will forward to MM. Henry a screen, by means of which they will determine the time (t), expressed in minutes, required to form impressions of stars of the eleventh magnitude, starting from the ninth magnitude of Argelander. Then, for all observers who will be furnished with an identical screen, the ratio $\frac{40}{t}$ will be the factor by which the time of exposure necessary to give the eleventh magnitude is to be multiplied, in order to obtain the stars of the feeblest magnitude for the chart."

If the Congress had done nothing more than adopt these two resolutions, the meeting would have been justified. They not only render possible, but really insure, the execution of both the chart and the catalogue. In addition, however, much good work was done, both as concerns the method of measurement of the plates and the arrangements for effecting the meridian observations of the "*étoiles de repère.*" There is, too, left on their minutes this further important resolution :—

"Concerning the method of reproduction of the stars for the chart, only purely photographical processes shall be employed, to the exclusion of all other processes which demand the intervention of manual labour."

The names of Captain Abney and M. Cornu are added to those of a previously existing Committee to settle the details of the process.

It is anticipated that the metallic screens will be distributed among the various Observatories in less than two months. In the meanwhile each Observatory is at liberty to proceed at once with the catalogue plates, care being taken that stars of the eleventh magnitude are impressed. In this sense, Admiral Mouchez's words are to be understood when he declared the work of the photographic chart of the sky commenced.

MAGNETIC ANOMALIES IN FRANCE.[1]

BY M. MASCART.

[*Translation.*]

THE first magnetic observations made in France by M. Moureaux, in 1884 and 1885, at not more than 80 stations, only enabled us to sketch the magnetic characteristics of the country in their broad features. Nevertheless, these first results were sufficient to detect certain anomalies, which are independent of the well-known irregularities which occur near rocks of volcanic origin, and which show no trace of the uniformity which is generally supposed to exist elsewhere. It was therefore necessary to multiply the stations in order to obtain a representation of the facts, which, if not absolutely correct, should be at least substantially accurate.

We therefore propose to cover France with a much closer network, which shall include about 600 stations properly distributed. Up to the present the north and the north-west, or, rather, the districts penetrated by the Chemins de Fer du Nord et de l'Ouest, are nearly completed, together with the region which extends southwards from Paris to the Loire. The number of stations is now over 200. The following is a description of the first results of these new investigations.

The map of the isogonal lines, in the district under consideration, discloses two principal anomalies, the one in Brittany, the other in the immediate neighbourhood of Paris. The first of these, discovered by M. Moureaux in the year 1886, has not as yet been fully studied. It is still necessary to observe at some points between Pontivy and Morlaix, and along the coast line between the mouth of the Loire and the bay of Douarnenez. Nevertheless, it is beyond question that a centre of disturbance exists near Pontivy, where the declination, which has been determined twice, with the interval of a year between the two observations, is less than at Ploërmel, which is 90 kilometres to the east. To the west of Pontivy, the isogonals are bent towards the north-west, and are drawn together near Morlaix. They next run south, or even south-south-east, thus roughly imitating the form of the coast-line of Brittany. It is possible that when the survey of this district is completed, the primitive rocks which occur in it will serve to explain the peculiarities which observation has brought to light. It is therefore unnecessary to discuss it fully at present.

The second anomalous region, however, is of special interest, on account of the nature of the soil. The attention of M. Moureaux was called to it by the results which he obtained at Chartres on June 22, 1889. Other observations, made both in this town itself and at various other places in the district, have confirmed the accuracy of the first measurements, and have led the observer to study the phenomena in detail in order to discover the magnitude and extent of the anomaly.

According to the provisional map of the true isogonals, the direction of the lines of equal declination only becomes approximately uniform towards the northern boundaries of France. Irregularities can be detected even in the latitude of Amiens, which extend as far as the Ardennes.

The isogonal of 15° 20′, which passed through Paris on January 1, 1890, instead of extending towards Orleans, bends to the south-south-east as far as Gien, then doubles suddenly on itself, running north-west to Houdan, which is west of Paris, and finally resumes a southerly course on the geographical meridian of Chartres. The isogonals, drawn for every 10′ of declination, have all the same shape, from the English Channel as far south as the network of stations at present extends—that is, to near Cosne ; and this conclusion is justified by a large number of concordant observations.

If the map of the true isogonals is compared with that in which the isogonals are assumed to be regular in form, we can deduce for every station the difference between observation and theory ; and if the points where these quantities have the same value are connected, we obtain a map of the isanomalous curves of the declination.

A zone, corresponding to an excess of declination, extends on this map from the shores of the Channel (Dieppe) as far as the Loire (Cosne), and increases in intensity towards the south. The excess is 14′ at Neufchâtel-en-Bray, 19′ at Nantes, 24′ at Chevreuse, 30′ at Gien, and 36′ at Cosne. The northern extremity of this zone extends towards the east to the neighbourhood of Laon, where the excess is still 7′. Immediately to the west of this zone there is another, almost symmetrical with it, which corresponds, however, to declinations of less than normal value. Like the first, it increases in intensity towards the south. The discordance is −6′ near the mouth of the Seine, −8′ at Evreux, −10′ at Dreux, −13′ at Epernon, −18′ at Orleans. Thus, contrary to all expectation, the declination is less at Orleans than at Montargis and Gien, less at Epernon than at Paris.

The facts are consistent with the view that the north pole of the needle is attracted from both sides towards a line which passes through or near Fécamp, Elbeuf, Ram-

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[1] This article is a *résumé* of a communication made by M. Mascart to the British Association at Leeds.